Notes WESTEX 2
March 21, 1959

Check with Rossi with respect to the distribution of the minutes and also to find out what's been done in communicating with Bronk to the Russian Academy and similar representation might be made if COSPAR is going to have a continuing committee have to wait his report on that.

Could I summarize by saying that a continuous scanning both geographically and temporarly would give us a measure of the global metabolism for the compounds that are not so very difficult to detect by spectro-analysis and which are really quite crucial and these are carbo-hydrogen. If you get marked fluctuations not only in appearance but in intensity over different intervals we can really get a much more detailed picture of the planet geography, so to speak,which should certainly be possible to nail this down absolutely and give actually some characterization of it in terms of seasonal change and so on. We won't really know very much about the intimate life history of the biota there and for this, unless someone has a better idea than what I've been able to come up with we are going to have to come down and take samples.

Discussion was initiated by Davies who indicated something of the basic missions of the Jet Propulsion Laboratories. Originally JPL was a contractor to the Army Ballistic Missiles Agency and was transferred over to NASA. Their general role is to build a few prototypes prior to a later production design and at the present time they also give technical direction to the Atlas Program by Convair. They will generally subcontract for the first and second stages and they will do guidance and perhaps even build the last stage in their own shops. They are also immediately concerned with the payload and instrumentation.

They are anxious to get together early in the game so that any instruments can be built into the payload. The Atlas Vega program is underway with a design target of June 1960. At NASA headquarters a few working groups have been set up. Newell is in charge of most of these and one working group now in existence is Jastrow's on Lunar Exploration. Planetary working groups have not yet been organized. NAS will probable advise and promote but its role is receding with the development of NASA. It is not a review body at the present time.

Then proceding to the agenda, a number of items were taken up. WESTEX-1 was reviewed thoroughly and after considerable discussion in which Urey participated, these reports were adopted as faithfully reflecting the sense of the meaning. There was unanimous agreement that a contamination load of 10^8 organisms per missile, alive, represented a maximum tolerance and that every effort should be made to destroy any viable organisms. As it was finally put to Hibbs, the main point with respect to the moon was that the biologists would not criticize him if the sincere effort to minimize contamination was not entirely successful and a few spores were landed on the moon. However, very faulty design would be indicated and subject to great criticism if a single spore were landed on Mars. Venus is still questionable and until its temperature characteristics can be settled finally, it will be necessary to treat it with the same caution as Mars. We had some discussion with Urey concerning the possibility of local moisture on the moon, and as we agreed (1) that this probably did not afford an opportunity for indigenous evolution of life, and (2) that while it was still conceivable that there might be a habitat for life, this would necessarily be highly localized. I read the quotation from Blagonravov. No one present had any ideas, thoughts or hints of any kind whatsoever about the Russian program. Urey felt that the general tenor of

scientific work in Russia is so spotty that there would be some question whether we could rely on their decontamination efforts. We still face the serious question of how to come to terms with Russian scientists. Urey indicated that there would be some geochemistry meetings in Germany this coming August and he would endeavor to set up a Symposium on the origin of life in which he would make every effort to include discussions of planetary biology. He apparently was counting on Oparin to be present. Calvin indicated that there would be the next Biochemical [illegible] Congress in Moscow in 1961.

(2) Apparently no one will be going east to Cambridge and there is no prospect of direct communication with Rossi. After some discussion we we agreed that we should at least consult Rossi before making wide dissemination of the reports. However, certainly no one could be embargoed on indicating his own ideas.

(3) As another specific point [illegible] Davies suggested that JPL or some comparable vehicle representation should be on the COSPAR subcommittee which evidently would be appointed to look further into technical matters connected with sterilization. Aside from the assistance such a person could offer in the development of policy, it would be invaluable to have him come back so as to be able to interpret that policy in practice.

(4) "Washington Dateline" was the source of a statement that NASA was taking a hard look at the moon shots and the contamination question. No one knows where this leaked from although it must have been from the Jastrow meeting.

(5) Suggestion was made to give some publicity to the question of planetary contamination through the medium of submission of an article to Priroda among other scientific journals. (I might perhaps feel that it would be appropriate to wait to hear from the CETEX committee but I will consult them about it.)

(6) There was then some discussion of the possible ultimate use of germ-free animals although these were considered to be fragile.

(7) Urey raised the question whether micro-organisms would be capable of attacking some of the carbonaceous chondrites as substrates for microbial action. Unfortunately very few of these specimens are available and the Chicago Museum and the British Museum are not very cooperative. I should perhaps contact Harry Brown from this point of view. (I should contact Brown about getting some specimens) Urey also mentioned that white chondrites when heated turn black all the way through. Nothing is known of the chemical basis of this and it might have some relationship to the low albedo of the moon. He had some feeling that the stoney meteorites might have a different origin from iron meteorites since they have vastly different ages in years. He also thought it would be most worthwhile to do reflection spectra on chondrites

(8) Sinton (1) promised to ask for a set of reprints for further distribution

(2) Gave some data on the radiation temperature of Mars which apparently varies from about -70° C. to +40° from the ground. The air temperature may vary very much less.

(9) The main anomaly was the high absorption by Mars at higher wave length which foliage does not show. When, however, he looked not at the one micron band but at 3.67 3.56 and 3.43, he found that these

were relatively prominent. No such bans were found on the moon. Urey was quite convinced that this is evidence of life. He argues that if life had evolved on Mars there must have been oceans at some time. He calculates that the present amount of water would escape in some few thousands or millions of years and therefore it must be constantly replenished. Later he gave a model where this water may be trapped under glacial rubble at the equatorial bulge.

(10) Sinton also reports that the atmosphere of Mars certainly has no more methane or nitrous oxide than does that of the earth and may have none at all.

(11) Weaver suggested a scanning across Mars with narrow band filters. (He will evidently spend some time in thinking of design of possible scanning spectrometers. I wrote some sort of a design myself involving the use of filters or gaseous absorption cells and might want to do some reading and thinking about this.) The reflectances of organic compounds for various wave lengths should be looked into. The literature on this seems to be very limited. Their conclusion was that there was urgent need for processing of a reflectance spectra library in order to back up the analysis of planetary surfaces by probes. Models should be used which give the type of diffuse reflection which will be measured

(12) Urey then quoted his version of the evolution of planetary atmospheres on Venus the earth and Mars. On Venus, oxygen could not escape and is trapped with carbon owing to the absence of photo of photochemical or photosynthetic reduction. Also there can be no water, the hydrogen having escaped.

(13) He referred to confirmatory observations by amateur astronomers of the haze at the crater Alphonsus.

(14) Mintz reviewed some aspects of the Venus atmosphere above the reflecting layer. Surface temperatures of 500° K. have been indicated by radio measurements at 3.5 and 9 centimeters. There is, however, a contradiction since this would imply a very deep atmosphere which contradicts certain other observations. He would feel that there would be fairly even temperatures in the gradient both from the equator to the pole and from the dark to the light sides.

(15) With regard to communications, Hibbs found that a T-V line would cost some $30,000 whereas a telephone line would be relatively inexpensive, and they are setting one up for contact with NASA.

(16) Next meeting Sunday May 3, at Pacific Grove, Send post-card to Van Niel by April 15th.

(17) What can be accomplished by reflection spectrometry of Mars would be ultimately a study of the geographic and temporal variation in the prevalance of organic compounds: the CH bonds and more optimistically CO and NH. These would tell us something of global metabolism on a large scale. The radar roughness experiment would be much less conclusive. The interpretation of the Moon measurements is that there is essentially specular reflection.

(18) Order seven copies of the space handbook and send to Urey, Novick, Stent, Weaver, Thomas, van Niel and Sinton.

(19) Sinton will arrange to send us reprints.

(20) Landings on Mars may be scheduled for about 1965. This gives us until the end of 1962 to complete our specifications for biological

experiments.

(21) Inquire what action if any Bronk had taken, and whether he will reiterate a message to the Russian Academy of Sciences.

(22) There does not seem to be very much more promising from remote approaches besides reflection spectra and these have to be validated to a larger extent from surface observations. (Write to Sagan about this.)

(23) Premature exploration by manned vehicles pose a serious threat to biological study.

(24) Under present design the last stage will accompany the payload in orbit, being separated only by slight spring action.

(25) Towards the end of the discussion there was mainly a concern of what might be gleaned from a Mars approach. Sinton will send a somewhat more considered account of what possibly could be gotten from that. He seemed to feel that there was not a great deal or that it was questionable how much more a probe would give than more terristrial observations. (I suspect myself that this may be somewhat colored by his own predilection for telescope astronomy.)

(26) What are the electric fields in the upper atmosphere? Perhaps should question Bracewell about this. The upshot is that as far as immediate experimental work is concerned we should:

(1) set up for more detailed background on reflectance spectra on terrestrial objects, and

(2) microbial attack on these meteorite samples.

I should consult Ginston to see if he can give me any more interest or background on the reflectance spectra question. One wonders how much might be done from microwave analysis.

(27) For the Mars probes one is talking about a 300 pound payload of which 30 to 50 pounds may consist of instruments.

(28) Should stress to Rossi that nothing should be considered accepted as a formal resolution until it had been possible to distribute it beforehand in duplicated form. This can now be considered to be the case for the WESTEX-1 series of statements. If nothing else this was an important reason to convene the group together otherwise, we could newer be quite certain whether this was a considered judgment on the part of the entire group.

(29)Calvin, Marr and myself will be a subcommittee to look into subcontracting infra-red reflectance measurements

(30) motion picture showed handling of the payload. What stability?

31: They had felt that cosmic rays were the chief obstacle to panspermia.

Preliminary studies on Planetary Biology

At the request of Prof. Bruno Rossi, acting for the NAS Space Science Board, Prof. Joshua Lederberg convened a group of biologists at West Coast universities to review some problems of policy in the space research program (particularly biological contamination), to suggest some tangible experimental approaches to the detection of life on other planets, and to stimulate broader interest on the part of biological scientists generally so as to evoke further proposals for experimentation. This groupphas met on two occasions so far: February 21, 1959 (Stanford) and March 21, 1959 (Jet Propulsion Labs., Pasadena); a next meeting is scheduled for May 3, 1959 (Hopkins Marine Station of Stanford University, Pacific Grove). In addition to the members listed below, we have had representatives from the Stanford Research Institute, and from JPL and NASA (A. Hibbs and R. Davies). We have reported to the NAS through Rossi and Odishaw at the Washington office, and to NASA via Hibbs and Davies, and also by letter to Jastrow (on the lunar exploration working group at NASA headquarters). We have also reported to COSPAR (CETEX) by correspondence with Hughes and with Peter Alexander. A jargon, self-indicated name for our group has been "WESTEX". Its university members have been: (1) signifies one mtg. attended.

U. of California (Berkeley):		U. of Cal. (Davis):		U. of Oregon:	
Calvin	Chemistry	Marr	Bacteriology	Novick	Biophysics
Mazia(1)	Zoology			Cal. Inst. of Technology:	
Stanier(1)	Bacteriology		(La Jolla):	Horowitz	Biology
Stent	Virology	Urey (1)	Chemistry	Stanford University:	
Weaver	Astronomy			Van Niel	Microbiology
				Kraaskopf	Geochemistry
				Lederberg (recorder)	Genetics

The composition of the group therefore reflects a balance between diversity of interest and locale and compactness of size, and convenience of assembly. Doubtless we could profit by special talents of other members, but the group should not be enlarged to the point where frequent and easy assembly becomes difficult, or where frank and casual discussion is inbibited.

While many members doubtless came to the first meeting with some sense of amusement and frivolity, it is obvious that the group as a whole is anxious to devote itself to tackling the problems of biological exploration with earnest endeavour. Many of its members are recognized as leaders in their own scientific fields, and in their academic communities. They have many other responsibilities. Nevertheless, there was unanimous enthusiasm for the continuation of its studies, and for meetings at relatively frequent intervals for mutual education and discussion. For this purpose, and to bridge the gap between exploratory discussions and preliminary experiments on one side, and tangible proposals and instrumentation for payloads on the other, we will require a substantial measure and continuity of financial support. In this proposal, several grades of support are indicated for more and more comprehensive activities. While the last and largest items might be deferred pending the elaboration of more explicit proposals, we should have prompt verification of support for our current discussions.

Minutes of Westex's first meetings are appended. The first meeting (Westex-1) was mainly devoted to problems of policy in celestial contamination, in view of urgent needs for the CETEX-COSPAR meetings. In brief we concluded that a basic policy of rigorous decontamination of space probes was both essential and feasible, -- modern methods of sterilization having been overlooked in other discussions. At the second meeting, this policy was reaffirmed. We then heard from Sinton on infra-red reflection spectra of Mars, which have furnished virtually conclusive evidence for 'vegetation'. We are digesting a number of ideas for improving the quality of this type of information from 'safe' (viz. distant approaches), and this will doubtless be the main topic for the near future. Finally we have in mind the careful preplanning of

experiments based on 'soft landings', especially on Mars, predicating these for about 1965. This will allow somewhat over two years for decisions on the most efficient types of experiments, leaving an equal length of time for the development and testing of the corresponding instrumentation. This timetable while not oppressive still does not allow for an indefinite waste of time. If properly supported, perhaps this might be one program that can be pursued with reasonable diligence and care rather than frantic haste. There is of course the possibility that the schedule may be accelerated (or delayed) by unforeseen technical factors, or by the pressure of international competition. Specifications for the vicinal probes are perhaps already under substantial pressure of time.

Proposal: Westex (A)

Travel, Communications [redacted] for continued meetings of the Westex Group. $ 6,000 per year.

This is based on holding about 10 meetings per year at various locations. There is a substantial advantage in meeting at different places, not only for the convenience of its peripheral members, and to help assure their attendance, but also to make further contacts with other local scientists.

While the travel costs are reduced by our regional grouping, this rather facilitates our meeting more often and more effectively for a given appropriation. In addition, there are substantial telephone charges for related business-- the more so to make the most effective use of frequent meetings. It is likely that not every member will be able to attend every meeting. On the other hand we would profit greatly by being able to invite occasional distant 'consultants'-- e.g. Fred Sinton at Westex-2. Admittedly, the development of a field as novel (in the U.S.) as astrobiology requires some expense for just the education of the workers who may participate in it.

If permissible, some of these funds (actually an insubstantial sum) should be available for the purchase of reference materials for the use of Westex members. On the other hand,at this stage, the time of Westex members is made available without cost other than expenses.

Westex (B) $7,500 one year only.

Publication of background information; "Handbook of Planetary Biology"

From the first discussions with Dr. Rossi, it has been evident that a critical requirement for the participation of U.S. biologists in space research is the collection of background information in a convenient form. This would include resumés of the Westex and 'Eastex' meetings, the essentials of present and prospective vehicle capabilities, and the environment (in the vehicle) for experimentation, and a critical discussion from the biologist's standpoint of available information on the environemnt of interplanetary space and the various planets. Most of this information can be found in the astronomical and other literature, but we know from our own experience how difficult it is for a biologist,who has not given much previous thought to extraterrestrial science, to acquire this background. For example many of our colleagues still believe that the capability for planetary probes is decades away (which, hoepfully, is not true) so that it would be pointless for them to attend to this challenge. While some member of CETEX may be able to rob the time from his other duties to prepare such resumés, this really is a substantial job, and there is some problem in finding a sufficiently informed enthusiast to do the work. Fortunately Mr. Carl Sagan may be available for some months this summer, and perhaps again after he completes his dissertation in astronomy (planetary atmospheres) at the Yerkes Observatory. A proposed budget would be $4,000 for a (part-time) salary to Mr. (later Dr.) Sagan as consultant to Westex, plus $3,500 for incidental costs in secretarial work, duplication, travel, reference materials. Mr. Sagan might have several functions: a) in the preparation of the consolidated reports of Westex (and, with their approval) Eastex for, perhaps, journal publication; b) as an adviser to Westex, particularly in the review of existing literature, and c) in the preparation of the more extensive handbook. This might have some 60-100 pp. The means of its dissemination is open to further discussion -- either informal distribution to some few hundred leading scientists, as a mimeographed bulletin, or publication by NASA or by a commercial publisher (which should not be difficult to arrange, if this is the best course).

Westex (C) Exploratory experiments. $10,000 -- 50,000

The design of payload instruments will have to be backed up by a substantial amount of laboratory work, since the analytical methods are limited by restrictions on weight, closeness of approach, automation, and the communication bandwidths. For example, there is relatively little published information on infrared reflection spectra of various materials, as would be comparable to Sinton's measurements on Mars. Before any member of Westex commits his owntime and resources, to the point of preparing a detailed proposal of laboratory work involving large scale support, some exploratory observations should be made in one or another laboratory, or perhaps most conveniently on a subcontract basis with some institution such as Stanford Research Institute. For example, the evaluation of Sinton's work, and its use as the basis for vicinal probes, would be greatly facilitated by the measurement of diffuse reflection spectra from model spheres coated with various substances (e.g., cellulose; pastes of photosynthetic bacteria). Unquestionably many similar questions will arise (and have arisen). It would be most expeditious if funds were available to help support exploratory trials on points which arise in our discussions. While, in terms of this proposal, these would be administered by one responsible grantee (Stanford University) it is understood that these would be available for expenditures at other institutions as will give the most expeditious results in these preliminary stages. Further development will be on the initiative of a scientist who undertakes the responsibility for pursuing a particular program, and will prepare his own budget request for this.

The scope of these explorations will probably be influenced by the funds that NASA is prepared to offer for them. I can visualize effective use of at least $10,000 per year for a rather limited scale, or perhaps $50,000 if we can have some leeway in purchase of equipment and in preliminary instrumentation towards payload designs. Any more extensive expedditures should certainly be made on the basis of explicit projects, following these explorations. These are, of course, uncommonly expensive as they require the development of new equipment modifications; in any case, even commercially available equipment in the particular field of molecular spectroscopy is far from inexpensive.

D. Possible Stanford projects. 1960 ff.

This is a preliminary statement concerning the possible continuation of Sagan's present work after he completes his dissertation at Yerkes. He has been studying the spectra of the major planets and is interested in the identification of some lines with more complex molecules, e.g., amino acids, as must be expected to be formed photochemically on the basis of Miller's experiments. He is interested in further model experiments on the extent of organic accumulation, especially in gravitational fields--which has an important bearing on the possibility of organic sediments, e.g., under the Jovian oceans. This is precisely the same work as is needed to support experimental designs in ultimate probes to these planets, and it fits very closely with the Martian models, in which he is no less interested. I would propose to use the opportunity of Sagan's work as a consultant to Westex (proposal B) to lay the groundwork for a more detailed proposal. This would doubtless appear as an application over his own signature as responsible investigator, though I would support this in every way possible.

This statement is made to illustrate one way in which interim support for our group can help to build up momentum for research in planetary biology.

As concerns my own participation, apart from recording these conferences and exciting the interest of my (sometimes still diffident) colleagues in biology, I would feel most at home in any personal laboratory work in contact, rather than vicinal, experiments. If we develop an international policy of space exploration that assures an uncontaminated field of exploration, I should be interested in developing techniques of cultivation and assay for use with soft landings. In view of the indicated timetable, I would not need special financial support for another two or three years, especially if some exploratory resources are available from proposal C.

J. Lederberg